
On Early Trade

Author(s): V. Sarianidi and Philip L. Kohl

Source: *Current Anthropology*, Vol. 20, No. 1 (Mar., 1979), pp. 165-167

Published by: [The University of Chicago Press](#) on behalf of [Wenner-Gren Foundation for Anthropological Research](#)

Stable URL: <http://www.jstor.org/stable/2741885>

Accessed: 10/12/2010 10:46

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=ucpress>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



The University of Chicago Press and Wenner-Gren Foundation for Anthropological Research are collaborating with JSTOR to digitize, preserve and extend access to *Current Anthropology*.

On Hominid Sexual Dimorphism: Reply

by MILFORD WOLPOFF

Department of Anthropology, University of Michigan, Ann Arbor, Mich. 48109, U.S.A. 14 VII 78

Although the commentary approach to CURRENT ANTHROPOLOGY publications has usually been a useful vehicle for the presentation of serious discussion, the spectre of continued clear-cutting of our Northwest Coast forests leads one to question whether these discussions should include points that could have been clarified through a more careful reading of the paper in question or through a query sent by mail. The comments presented by Williams, Sciulli, and Poirier (CA 19:636–37) provide an example and moreover presuppose an extraordinary degree of ignorance, if not dishonesty, on my part in the utilization of canine-socket measurements to estimate transverse canine breadths. The authors claim that insufficient data were presented to show that this estimation is accurate for the australopithecine sample, that the resulting regression of socket on actual canine breadth may have been ineptly applied, and that eliminating part of the sample “results in essentially unimodal curves” (a contention unsupported by any statistical test).

I offer the following points in response:

1. In stating that the transverse canine-breadth measurement is inevitably maximum in the cervical region and homologous

and analogous for the nonhuman primates (CA 17:580), I had hoped to make it clear that this condition applies to all the species concerned without listing them separately.

2. The least-squares regression formulae and the appropriate data regarding their fit are as follows: maxillary canine = $1.01(\text{socket breadth}) - .13$ ($r = .990$); mandibular canine = $.94(\text{socket breadth}) + .6$ ($r = .996$). For the maxillary sample ($N = 8$), the average absolute difference between the socket and canine breadth measures is .11 mm, while the average absolute difference between the canine estimate and actual breadth is .07 mm. For the mandible ($N = 10$), these differences are respectively .12 and .06 mm. Had the authors requested these data from me by letter, their discussion of “what the data might have shown” would have been unnecessary and a number of trees could have been saved.

3. In any event, these absolute errors are close enough to justify a direct estimation of transverse canine breadth by socket breadth. Alternatively, their comment simply could have been based on the assumption that I attempted to see if my procedure was accurate before I used it, although this too would have rendered it unnecessary.

4. Finally, there is no question that removing some data from a small sample makes it more difficult to interpret. If the authors are interested in pursuing this line of reasoning, they might try to remove all measurements ending in an odd number, or perhaps those with specimen numbers with more than three digits, and report on the effects of these procedures. I look forward to hearing about their results.

On Early Trade

by V. SARIANIDI

Institute of Ethnography, Academy of Sciences, U.S.S.R., Dm. Ulanov 19, Moscow 117036, U.S.S.R. 10 III 78

Вопросы древней политэкономии все более становятся объектом углубленного изучения археологии, свидетельством чего является симпозиум 1972 г. в г. Ленинграде, посвященной торговле и обмену (материалы которого были опубликованы в специальном номере журнала «Краткие сообщения ИА АН СССР» No. 138). Этой же теме посвящена в высшей степени квалифицированная статья Kohl (CA 19:463–75), в которой с исчерпывающей полнотой обобщены известные данные, и намечены свои собственные принципы дальнейшего исследования этой проблемы. В целом основные тезисы статьи полностью отвечают современному уровню наших знаний, что разумеется не исключает отдельных спорных положений, как например допущение подневольного труда ремесленников Тепе Яхья слоя IVB1 или некоторых модернистских формулировок.

Вместе с тем многие спорные положения обусловлены общим недостаточным уровнем изученности и состоянием науки и в первую очередь отсутствием твердых, объективных критериев. Специфика археологии такова, что она устанавливает факт проникновения предметов, чуждых определенной культуре, но как правило не выявляет причины их—в результате ли миграций, обмена, торговли, военного захвата и т. д. Наглядным примером тому могут служить современные работы в Бактрии эпохи бронзы, где в результате археологических раскопок были обнаружены отдельные предметы глиптики, художественной бронзы и даже керамики, находящиеся явные иранские параллели. Было высказано мнение о их импортном происхождении, что полностью соответствовало состоянию изученности Бактрии того начального периода. Однако, последующие беспрецедентные по масштабам для

конца XX в. грабительские раскопки многих тысяч могил Бактрии эпохи бронзы, выявили колоссальный погребальный инвентарь, включающий металлические зеркала, топоры, сосуды, булавки с фигурными навершиями, каменные колонки и виллообразные орудия гиссаро-сиалковского типа. Все эти материалы также обнаруживают бесспорные иранские аналогии, однако их массовое количество на чисто исключило предположение о возможном импорте и наоборот дало веские основания утверждать их преимущественно местное производство. Более того стало очевидным, что заселение бактрийской равнины древними земледельцами произошло за счет инвазии через северо-восточный Иран больших групп племен, принесших с собой свои собственные навыки в материальной культуре. Все это не исключает обменные и возможно торговые связи не только с северо-восточным Ираном, но и Эламом; свидетельством последнего являются цилиндры месопотамского типа, встреченные в Бактрии.

О сложности исторической интерпретации археологических наблюдений могут свидетельствовать и печати, определяемые как безусловные символы личной собственности. Между тем из тех же разграбленных могил Бактрии происходят десятки однотипных, до деталей повторяющих друг друга металлических печатей, что намекает не столько на личную, сколько на коллективную (семейную клановую и т. д.) собственность. Думается, что и сами печати II тыс. до н. э. более связаны еще с культовой символикой былых общинников, чем с нарождающимися частнособственническими отношениями. Косвенным свидетельством может служить одно рядовое детское погребение с Улуг тепе (Туркменистан), в котором помимо обычной погребальной керамики находилась массивная металлическая печать.

Очевидно, что выработка объективных критериев является первоочередной задачей в разработке палеоэкономических систем мировой археологии.

Вторая важная проблема касается терминологического определения понятий «обмен» и «торговля». Хотя Kohl считает, что деньги не являются основным показателем

торговли имеется и иная точка зрения, согласно которой обмен перерастает в торговлю лишь постепенно с момента возникновения первых вариантов всеобщей эквивалентной формы стоимости—денег. Древнейшие, или иначе первобытные деньги, еще не имеют значения всеобщего платежного средства. Они применимы лишь к определенным товарам и в этом заключается их главное отличие от денег классовых обществ. Отделение ремесла от земледелия стимулировало превращение ремесленной продукции в товар, идущий на продажу что в конечном счете и приводит к развитию первобытной торговли. Всем этим условиям отвечает Тепа Яхья периода IV, когда изобилие каменных сосудов типа Intercultural Style сочетается с протоэламскими табличками и лишь особые причины помешали ему перерасти в городок.

В этот переходный период первобытной торговли роль денег могут играть различные эквивалентные товары, но развитие металлургии способствовало формированию первобытных денег, выраженных преимущественно в металле. Наряду со слитками, как считают, это могли быть и металлические изделия, потребительское значение которых нередко умышленно нарушено, наподобие миниатюрных до 5–7-см металлических топориков из Гиссара и Сялка, которые теперь известны и в могильниках Бактрии. Как предполагают, возможно ту же роль эквивалентных денег играли металлические, всегда строго однотипной формы «булавки» в виде стержня, один конец которого раскован, а другой имеет каплевидное утолщение, широко распространенное в IV–II тыс. до н. э. в Средней Азии и смежных областях. Не исключено, что подобные металлические изделия играли в этом обширном регионе в домонетный период роль первобытных денег вплоть до появления собственно металлических денег, знаменующих собой наступление подлинной торговли классовых обществ.

[Problems of the political economy of ancient times are more and more frequently becoming the subject of intensified archeological investigation, as is attested by the symposium held in 1972 in Leningrad on trade and barter (the proceedings of which appeared in No. 138 of *Kratkie Soobshcheniya IA AN SSSR* [Brief Announcements, Institute of Archeology, Academy of Sciences of the U.S.S.R.]). Devoted to the same theme is Kohl's (CA 19:463–75) thoroughly researched article, in which he presents an exhaustive analysis of the known data and sets down his own guidelines for further research on this problem. On the whole, the main theses of the article entirely satisfy the requirements of our present level of knowledge. There are, of course, certain disputable points, such as, for example, his assumption of forced labor on the part of the artisans of Tepe Yahya IVB1 and certain modernistic formulations.

At the same time, Kohl makes many questionable statements which are attributable to the insufficient level of previous study and the limited state of the art, primarily due to the absence of firm objective criteria. It is the nature of archeology that, while it establishes the fact of penetration by objects alien to a particular culture, it does not, as a rule, bring out the reasons for their presence, be it migrations, barter, trade, military conquest, or whatever. A graphic example of this is offered by modern work on the Bronze Age in Bactria, where, as a result of archeological excavations, engravings, bronze works of art, and even ceramics with obvious Iranian parallels have been discovered. At first it was believed that these originated as imports, a belief which corresponded perfectly to the state of knowledge of Bactria in that initial period. However, subsequent excavations, on an unprecedented scale, of many thousands of Bronze Age Bactrian graves in the latter part of the 20th century have disclosed a colossal funeral inventory which includes metal mirrors, axes, vessels, pins with figured heads, stone columns, and forked implements of the Hissar-Sialk type. All these materials undeniably suggest Iranian analogies, but their massive quantity entirely excludes the possibility of their having been imported and, moreover, provides telling arguments in favor of predominantly local

production. Furthermore, it has become obvious that the settlement of the Bactrian plain by ancient agriculturalists took place through invasion from northeastern Iran of large groups of tribes that brought with them their own skills in material culture. All this does not exclude the possibility of barter and of trade ties with not only northeastern Iran, but also Elam; cylinders of the Mesopotamian type found in Bactria provide evidence of the latter.

Another indication of the difficulty of historical interpretation of archeological observations has to do with seals, which have been defined as unequivocal symbols of personal property. These same plundered cemeteries of Bactria have yielded many metal seals of the same type, similar to the last detail, implying not so much personal as collective (family, clan, etc.) property. It seems to me that even the seals of the 2d millennium B.C. are connected more with the religious symbolism of past members of primitive communes than with emerging private property relations. Indirect evidence of this is furnished by an ordinary child's grave at Uluk Tepe (Turkmenistan) in which, in addition to the usual funeral ceramics, a massive metal seal was found.

Obviously, the working out of objective criteria is a problem of the first rank for world archeology with reference to the development of paleoeconomic systems.

A second important problem concerns the definitions of the concepts "barter" and "trade." Although Kohl considers money not to be a basic indicator of trade, there is another point of view which maintains that barter develops gradually into trade only after the appearance of the first variants of a universal equivalent form of value, money. The most ancient or primitive money did not yet have significance as a universal medium of payment. It applied only to particular goods. This is the principal distinction between primitive money and the money of class societies. The separation of crafts from agriculture stimulated the conversion of a craft product into a commodity to be sold, which ultimately led to the development of primitive trade. All of these conditions are met by Tepe Yahya in Period IV, when an abundance of stone vessels of the Intercultural Style type coincides with proto-Elamite tablets; only special conditions prevented it from growing into a small town.

In this transitional period of primitive trade, the role of money may be played by various equivalent goods, but the rise of metallurgy facilitated the development of primitive money mainly in the form of metal. In addition to ingots, it is believed that this money could also have taken the form of metal articles whose original practical significance was often deliberately altered, as, for example, in the case of 5–7-cm-long miniature axes from Hissar and Sialk, which have now also been found in the cemeteries of Bactria. It is conjectured that the metal "pins," shaped in the form of a rod with one end beaten flat and the other having a droplet-shaped bulge, which were widely distributed in Central Asia and adjacent areas in the 4th to 2d millennium B.C., may also have played the part of equivalent money. It cannot be ruled out that similar metal articles were used as primitive money in this vast region right up to the appearance of coinage proper, which signified the beginning of the genuine trade of class societies.]

Reply

by PHILIP L. KOHL

Department of Anthropology, Wellesley College, Wellesley, Mass. 02181, U.S.A. 15 VII 78

I thank Sarianidi for his thoughtful comments on my paper. His own exceptionally productive work in northern Afghanistan graphically illustrates the way in which former conceptions must be revised with new discoveries and the fact that vast and important regions of the Near East and Central Asia, such as

the southern Bactrian plain, have received insufficient archaeological exploration, much less intensive excavation and survey. The mindlessness of the assertions of some "new archaeologists" (e.g., Leone 1972:26) who proclaim that we already know the major outline or facts of prehistory and must now proceed with the remaining problem of analysis and theoretical interpretation is demonstrated by Sarianidi's work, as well as by the results of excavations by Western scholars in Iran that I attempted to summarize in my article. Theoretical analysis and the collection of new data must proceed simultaneously if our understanding of cultural evolution or even determination of "laws" of cultural processes is to advance. The current de-emphasis on basic culture-historical reconstructions which prevails in the new, contemporary archaeology is singularly misguided and unfortunate.

This point is raised by Sarianidi when he observes that earlier interpretations of finds from Afghanistan as imports from Iran must be revised in the light of new data. As a result of his excavations it seems more likely that these metal and stone objects, which occur in great quantity, were locally made and that their presence must be explained differently. However, I question Sarianidi's interpretation of a tribal migration from Iran for the same reason: the prehistory of Iranian Khorasan, the source of the postulated migration, is almost totally unknown. Until such work is carried out and a detailed analysis of the relative chronologies and material cultures of both regions is made, I am not persuaded by this interpretation. Historically, we know that Bactria was an exceptionally rich and fertile area. Current work farther south in Pakistan by French investigators at Mehrgarh, where about 10 m of aceramic deposits have been discovered stratified beneath levels radiocarbon-dated to the 6th millennium B.C., is revealing the Neolithic backdrop to the Harappan civilization. That is, it is becoming clear that communities in Central Asia and along the Indus evolved locally, as well as, undoubtedly, at times received peoples migrating from other areas. It is inconceivable to me that such a local evolutionary record cannot be traced for

Bactria; our current understanding again only reflects the lack of sufficient archaeological work in the area. It may even be possible someday to reverse the directionality or causality and demonstrate that metallurgical and other technological and cultural developments in eastern Iran and Afghanistan were fundamentally important for the evolution of communities farther west.

Sarianidi's point about distinguishing trade from exchange and associating the appearance of primitive money with the former is well-taken. It raises the exceptionally critical problem of the determination of value on the basis of material remains. I have no answer to this problem but feel that the best chance for such a determination will be through a comparative and painstaking analysis of materials from cemeteries that have been sufficiently excavated; comparative data now exist from Shahr-i Sokhta, Shah-dad, and Hissar and, slightly later, from Bactria. If materials are analyzed according to provenience, quantity, and association at each site, as has recently been initiated at Shahr-i Sokhta (Piperno 1977), and compared with one another, some determination may be possible. It then, of course, would be fascinating to make a further comparison with Ur and other sites in Mesopotamia where we also can utilize textual sources, particularly now the rich archives from Ebla (M. Tosi, personal communication), to learn how proximity to source areas and other possible variables, such as sociopolitical considerations, affected the relative prices of different goods. I also must add that analogies based upon the ethnographic record make me confident that primitive money, as discussed by Sarianidi, was present throughout southwestern Asia in the mid-3d millennium B.C. (and probably much earlier). Thus, the use of the term "trade" seems to be justified.

References Cited

- LEONE, MARK. Editor. 1972. *Contemporary archaeology*. Carbondale: Southern Illinois University Press.
PIPERNO, M. 1977. "The graveyard," in *La città bruciata del deserto salato*, pp. 115-48. Venezia-Mestre: Erizzo.

On the Alleged Diffusion of Hindu Symbols to Mesoamerica

by CHARLES D. CHEEK

Proyecto Arqueológico Copan, Apartado Postal 1291, San Pedro Sula, Honduras. 20 VII 78

Mundkur (CA 19:541-68) demonstrates in a very scholarly way that the attempt to find correspondences between Mesoamerican and Hinduized Asian religious symbols rests on a superficial and inaccurate knowledge of the historical and iconographic nature of the Hindu material. He uses the two ancient weapons of antidiffusionists: the demonstration of time differentials in the appearance of a trait in donor and recipient cultures and the demonstration that the total context and configuration of the traits has not been considered.

I am mostly convinced by his arguments. Because of the nature of the diffusion problem itself, however, the article is inconclusive. It will elicit two kinds of responses. Antidiffusionists will praise it as support for their view, and diffusionists will thank the author for pointing out their deficiencies and proceed to use the new and better understanding of the material to sharpen their discussion of parallels.

Rather than focusing on specific diffusionary problems, those interested in such topics might find it useful to look at broader problems, such as the kind of diffusion that takes place between societies of different or the same level of complexity. For

example, in discussing the diffusion between India and Southeast Asia, Mundkur is describing cultural imperialism between Neolithic and more complex societies. Contact between India and Mesoamerica must have had a very different context, no matter how many or how few people came to the New World. I would hardly expect to see the Southeast Asian pattern repeated in Mesoamerica, as he seems to suggest at one point. Focus on the structure of the contact situation is not a new idea, but a broad-based comparative study should lead to some general propositions about the process of diffusion. This orientation will take us away from the study of diffusion per se and closer to a more general study of culture change.

In closing, I would challenge two statements about the beliefs of Mayanists. I do not agree that there is "extraordinary disagreement" among Mayanists about the identity of the figures on Maya stelae: the majority think they are historical personages. Secondly, I do not agree that the Maya did not have a pantheon during the Classic. Given comparative anthropological data, a society at the level of the Classic Maya "should" have a defined pantheon, and many scholars believe that one can be defined.

by JACQUELINE DE DURAND-FOREST

15, rue Lakanal, 75015 Paris, France. 6 IV 78

Parallèlement aux sciences dites exactes, la linguistique nous a familiarisés avec la notion de structure. Même si les autres sciences humaines n'en ont fait leur profit que plus tardivement,